Faraday's Discovery of Electromagnetic Induction

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On 29 August 1831 Michael Faraday discovered the induction of one current by another in his famous induction ring experiment familiar to every student of physics (Fig. 1).† It is no exaggeration, I think, to say that this is one of the few really great experiments in the history of science. Not only did it serve as the starting point for the development of the electric power industry in the nineteenth century, but it also stimulated Faraday to lay the foundations of field theory.‡ This being so, it is curious that the origins of this experiment have never been revealed. Almost without exception, the action of the induction ring has been described by means of concepts that Faraday formulated quite some time after his discovery, and to which he was led by it. Thus, most of us would explain this experiment in terms of the build-up and break-down of an electric field near an electrical conductor without being too perturbed by the fact that the concept of the electric field was (indirectly) the result, not the cause, of Faraday's experiment.

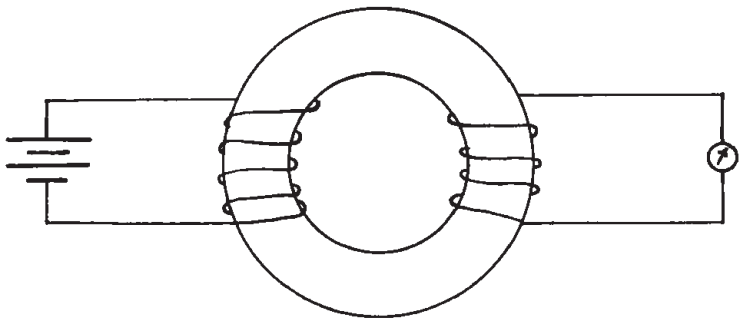

Fig. 1.

To a great extent the fault lies with Faraday. He merely reported his results in the first series of his magistral *Experimental Researches in Electricity*§ without letting anyone in on the facts and factors that had led him to devise his experiment. Almost everyone occupied with electrical science in the 1820's was conscious of the *possibility* of electromagnetic induction. When Faraday announced his success in obtaining it, he was highly praised but no one then or

† The experiment itself is described by Faraday in his laboratory Diary which was published, beginning in 1931, to commemorate this discovery. T. Martin, editor, *Faraday's Diary, Being the Various Philosophical Notes of Experimental Investigation made by Michael Faraday, D.C.L., F.R.S., during the Years* 1820–1862, 7 vols. (and index), London, 1931–1936, cited henceforth as *Faraday's Diary*. Only vol. 1 will be used so all page references are to that volume.

‡ L. Pearce Williams, ' Faraday and the Structure of Matter ', *Cont. Phys.*, **2** (1960), 97, and also, ' Michael Faraday and the Evolution of the Concept of the Electric and Magnetic field ', *Proc. R.I.*, **38** (1960), 235, and *Nature*, **187** (1960), 730.

§ M. Faraday, *Experimental Researches in Electricity*, 3 vols., London, 1839–1855.

since troubled to ask why he was successful where so many others had failed. Faraday, too, did not see fit, even in the Diary, to describe the ideas which led him to construct the iron ring and wind it the way he did. From 1821, when he began to investigate electromagnetism, until 1831, there are only a dozen references to electromagnetic phenomena and, of these, only a very few are concerned with electromagnetic induction. What, then, is the historian of science to do? Shall he simply report Faraday's experiment, and let it go at that? Given the lack of evidence, this seems both the safest and the most scholarly path to choose. Yet there is an almost overpowering temptation to try and find some explanation of the experiment, even if one must speculate and rely on conjecture for a good part of the account. This is what I have done here and, at greater length, in my forthcoming biography of Faraday. I have tried to take the twelve-or-so pieces that Faraday has offered and fit them into a fifty-piece puzzle. I can promise the reader that *my* speculations will be clearly labelled; the hints given by Faraday and those other indications of his thought provided by his correspondence will also be documented. When all is said and done, however, it should be recognized that this reconstruction contains only a few of the original stones and relies heavily upon the assumed logic of the evolution of Faraday's ideas.

There seem to be three separate phases in the development of the ideas that led to the induction ring experiment. In the first phase, Faraday had to wrestle with basic concepts of the nature of electricity and magnetism. From 1821 on he was engaged in an argument of fundamental importance with Ampère. His views differed significantly from those of his famous contemporary and during these years Faraday exerted himself to find some way of testing both his own and Ampère's views. His first real attempt to discover electromagnetic induction, for example, was conceived within the context of Ampère's theory and his failure stimulated him to continue upon his own way. This way led him to the rejection of electrical fluids and the conviction that electricity and magnetism were inherent powers of matter.

In the second phase of his development, Faraday was searching for a solution to a problem raised by his own concepts. The problem can be simply stated: electricity might be an inherent power of matter, but there was no doubt that some aspect of this power could be transmitted through the wires of a simple galvanic circuit. How could ' force ' be transmitted without the transmission of matter in which the force inhered? The solution to this problem came with Faraday's discovery of Fresnel's work on the undulatory theory of light and with his active investigation of the propagation of sound waves.

The third phase was the shortest and served as the immediate stimulus for setting up the induction ring experiment. This was the announcement in 1831 of Joseph Henry's creation of an electromagnet of hitherto unheard-of power.

One of the legacies Faraday received from Sir Humphry Davy was a certain scepticism about the use of imponderable fluids to explain phenomena. Davy was one of the earliest opponents of the caloric theory of heat, and he carried his distaste for imponderable agents over into the area of electricity. His electrochemical researches had convinced him that electricity was an inherent power of matter and that the use of fluids as mental aids only served to confuse matters. Faraday had been impressed by Davy's viewpoint, and, although he was not

ready in the early 1820's to commit himself wholeheartedly to an anti-fluid approach, he was suspicious of explanations based upon them. Even before his discovery of electromagnetic rotation in 1821 led him to challenge Ampère's theory directly, he had warned against the too facile acceptance of the fluid theory of electricity. ' There are many arguments in favour of the materiality of electricity,' he wrote ' and but few against it; but still it is only a supposition; and it will be as well to remember, while pursuing the subject of electro-magnetism, that we have no proof of the materiality of electricity, or of the existence of any current through the wire '.†

Electric fluids, it should be remembered, had been originally suggested to account for electrostatic attractions and repulsions. If charge were considered to represent the quantity of the electric fluids then, as Coulomb had shown, electrostatic forces could be represented by an equation completely analogous to the Newtonian law of gravity. Thus the charge Q in Coulomb's law of electrostatic forces, $F=(KQ_1Q_2)/r^2$, was the exact counterpart of the mass M in Newton's gravitational law, $F=(kM_1M_2)/r^2$. There could be no doubt of the materiality of ponderable matter represented by M and, by analogy, there seemed little room for doubt of the materiality of the substance represented by Q. Wnen the invention of the voltaic pile appeared to reveal a continual electrical action, rather than a momentary discharge, it was only natural to assume that this action was the result of a flow or current of the electrical fluids. This Volta had done and the term current passed rapidly into general usage. It was this rather uncritical and facile acceptance of the reality of a current to which Faraday objected.

This problem was complicated by Oersted's discovery of electromagnetism in 1820 and Ampère's suggestion that magnetism was the result of the flow of the electric current. Faraday was critical of imponderable fluids in general and he could not help but be suspicious of the complicated theory Ampère put forward. Since Ampère accepted the two-fluid theory of electricity an electrical current could not be visualized simply as the flow of a single substance through a wire. Instead, negative electricity flowed in one direction while positive electricity flowed in the other. The two electric fluids were the component parts of the luminiferous ether in which ponderable matter was immersed. The presence of a source of electromotive force caused the decomposition of the ether and the impulsion of the two electric fluids in opposite directions.‡

In his ' Historical Sketch of Electromagnetism ', Faraday did not pretend to know what a ' current ' was, but his remarks upon its effect are of great interest for they reveal a point worth noting. The ' current ', whatever it may be, according to Faraday, threw the wire conducting the current into a peculiar ' state '. And, Faraday continued, ' as it is in this state that the wire is capable of affecting the magnetic needle, it is very important for the exact comprehension of the theory that a clear and precise idea of its state, or of what is assumed to be its state, should be gained . . .'§

† Michael Faraday, ' Historical Sketch of Electro-magnetism ', *Ann. of Phil.*, N.S., **2** (1821), 195.

‡ See L. Pearce Williams, ' Ampère's electrodynamic molecular model ', *Cont. Phys.*, **9**, 1962.

§ *Ann. of Phil.*, N.S., **3** (1822), 112.

Here was the essential point of disagreement between Faraday and Ampère. To Ampère, electrodynamic effects were uniquely the result of the interactions of the two electrical fluids. Even permanent magnetism was explained in terms of electrical currents circulating around and through the molecules of ponderable matter. The source of the electrical fluids was the ether and the only role ponderable matter played was the rather vague one of providing the electromotive force. Faraday, on the other hand, was firmly convinced that electrical and magnetic forces were intimately connected with the particles of ponderable matter. Electromagnetism could not simply be the action of one current of electrical fluid upon another but must involve the particles of ponderable matter through which the ' current ' passed. There should be some kind of a ' state ' of these particles which permitted their inherent powers to come into play in such a way as to produce the effects of magnetism.

From 1821 until 1831, Faraday, when he had the time from other duties, sought to test Ampère's theory and to find evidence in support of his own. Out of these experiments came some of the elements of the induction ring experiment. Let us look at three experiments Faraday performed in his search. One was designed to test his concepts and two seemed to provide support for his opposition to Ampère's theory.

On 10 September 1822 Faraday tried to detect the ' state ' caused by the passage of current.

> ' Polarized a ray of lamp light by reflection and endeavouring to ascertain whether any depolarizing action exerted on it by water placed between the poles of a votaic battery in a glass cistern—one Wollaston's trough used—the fluids decomposed were pure water—weak solution of sulphate of soda and strong sulphuric acid. None of them had any effect on the polarized light either when out of or in the voltaic circuit, so that no particular arrangement of particles could be ascertained in this way.'†

This experiment is of considerable interest and should be seen as more than an illustration of Faraday's ideas in 1822. He was to return to it, or to a similar experiment, time and time again in the next two decades, so convinced was he that matter could not be indifferent to the passage of an electric current or the production of magnetic force. Finally, in 1845, his efforts were to be crowned with success when the plane of a ray of polarized light was rotated as it passed through a heavy lead glass in an intense magnetic field. All this, however, was far in the future in 1822 and, at this time, the negative result of the experiment served only to leave Faraday's judgment suspended between his and Ampère's theory.

A second experiment is of even greater interest. Here Faraday came within a whisker of discovering electromagnetic induction in 1825. More importantly it would have been a discovery based upon Ampère's theory, thus lending great support to the acceptance of it. The full experiment is not described in the published laboratory diary of Faraday but is to be found described in a manuscript at the Royal Institution of Great Britain in a volume catalogued as Davy's Laboratory Notebook. This volume contains entries by Davy, William T. Brande and Faraday; the section of interest to us is dated 25 November 1825. The description is brief.

† *Faraday's Diary*, 71.

" Two copper wires were tied close together, a thickness of paper only intervening for a length of five feet. One of them was made the connecting wire of a battery 40 pr. of plates 4 inch square in rather weak action and the ends of the other wire connected with a galvanometer. No effects however upon its needle could be observed consequently no visible proofs of induction by the wire through which the current was passing upon its neighbour could this way be perceived."

How tantalizing to the historian this brief paragraph is. What theory was in Faraday's mind at this point? It is, of course, entirely possible that he was merely trying what happened when a wire was placed next to another in which a current was passing and that he had no particular theoretical idea in mind. But, it can also be shown that this experiment can be deduced from Ampère's theory. There is little doubt that Faraday was intent upon devising experimental tests of the theory for one finds these tests scattered through the Diary and it seems plausible to me that this particular experiment also was such a test. In any case, it is worth seeing how Ampère's theory could predict electromagnetic induction and why, in 1825, Faraday missed noticing that he had come very close, indeed, to producing it.

Ampère's theory, it will be remembered, involved the complicated flow of positive and negative electric fluids in opposite directions through a wire. The result was a pattern of charge in the wire, as illustrated by Fig. 2. The luminiferous ether, which permeated the wires, was decomposed by the electromotive

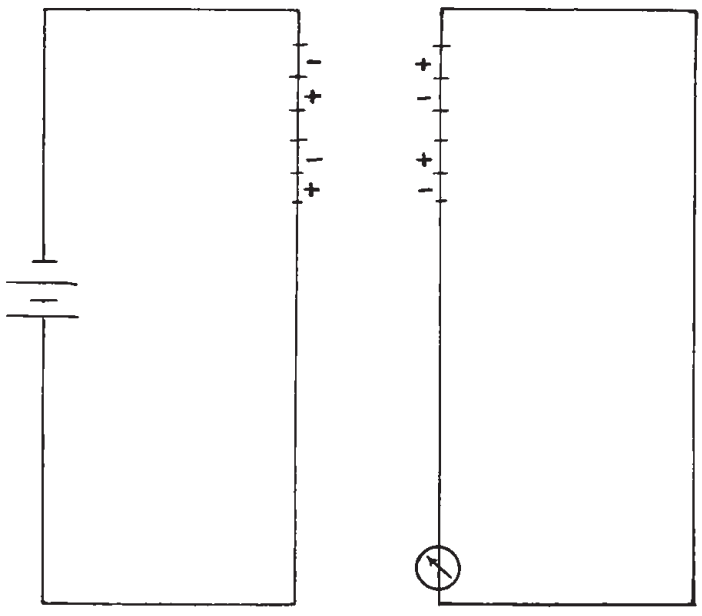

Fig. 2.

force of the battery in the primary circuit. A portion of positive current began to move in one direction while a portion of negative current started to move the other way. When the current was established in the wire, these portions of the two fluids continued to move past one another. When they met, they recombined to form the neutral ether, but this compound was again decomposed by the continued action of the electromotive force so that the positive and negative electricity continued their journeys. Thus, for some finite time,

however small, the opposite charges existed separate from one another. The wire would contain a pattern of charge as shown in the figure. This pattern, as a whole, would remain stationary although the fluids themselves would move. Here, then, was a ' state ' of the wire which Faraday thought essential and this ' state ' ought to induce a precisely similar ' state ' (with opposite charges) in a neighbouring wire. This induction should be an ordinary electrostatic one, but since it produced a pattern of charge precisely similar to that in the primary wire it might reasonably be expected to affect a galvanometer in the secondary circuit.

This explanation also serves to reveal why Faraday did not discover electromagnetic induction in 1825. Had Faraday connected the secondary circuit first and then closed the primary circuit, a momentary current would have been produced in the secondary. It requires no elaborate galvanometer to detect this current; a simple compass needle will do. It is inconceivable to me that Faraday, who repeated his experiments over and over, and who was alert to the slightest indication of any action, could have missed this current. So it must be concluded that he did not actually induce a current and this is precisely what we should expect from the theory of the experiment. If my explanation, or one similar to it, was the one Faraday was using, he would naturally close the primary circuit first so that the pattern could become established; then he would close the secondary circuit and look to see if the predicted induced pattern affected the galvanometer. He was looking for a steady effect, not a momentary one, and for an effect that should be produced only after the current in the primary had become established throughout the circuit. Hence, he saw no deflection of the galvanometer in the secondary circuit.

The third experiment was explicitly used by Faraday to challenge Ampère's theory. According to Ampère, a current-carrying helix was *exactly* similar to a permanent bar magnet. This Faraday showed was not true. As early as 8 September 1821 he had experimented on the location of the poles in a permanent magnet and in a helix. In a helix the poles were right at the end of the coil, but in the magnet the poles were some distance in from the ends. This appeared to contradict Ampère's theory that permanent magnets were merely the result of the alignment of his electrodynamic molecules for such an alignment should produce poles at the very end of the magnet. This criticism was trenchant enough to force Ampère to modify his theory and suggest that the molecules were not *really* all aligned, although he found it difficult to explain why. That, however, is not the point of interest for us. The location of the poles within the magnet appeared to Faraday to reinforce his idea that magnetism was the result of inherent powers of matter and this led him to ask himself a question, the answer to which was later to provide a fundamental part of his experiment in 1831. " Query the best form for a magnet, i.e. so that the pole may be nearest the surface: an oblate or oblong spheroid or a sphere or a very thick ring?"†

These experiments reveal Faraday wrestling with the basic questions of the nature of electricity and magnetism. They do not reveal any answers and indeed it can be said that Faraday did not possess any at this time. The one idea to which he clung was that electromagnetism, somehow, was associated with the

† Faraday's conviction on this point was the result of his work on electromagnetic rotations. See his ' On some new Electro-Magnetical Motions, and on the Theory of Magnetism ' in *Experimental Researches in Electricity*, vol. 2, 127 ff.

forces of matter and not, as Ampère insisted, with imponderable fluids separable from and independent of matter.‡ If this were so, the fundamental problem facing him was to account for the seeming transmission of force through space when the ponderable matter through which the force was transmitted remained stationary. The theory of imponderable fluids here was clearly superior, for one needed only to assume the passage of the fluids themselves to account for electrodynamic (the word is Ampère's) effects.

Faraday's Diary, letters and printed papers in the period 1821–1831 are mute on this subject. But we do know that his attention was directed to a similar problem during these years and there is, fortunately, a document written by Faraday just *after* the discovery of electromagnetic induction which will allow us to follow the evolution of his ideas.

In 1824 Faraday was named to a committee on the improvement of optical glass set up by the Royal Society. In 1827 he began actively to work at the Royal Institution on the nature of glass and the possible improvements that could be made in its manufacture. At the same time a series of articles appeared in the *Quarterly Journal of Science*, the semi-official journal of the Royal Institution, which Faraday read with interest. These articles were an English translation by Dr. Thomas Young of Fresnel's elementary theory of light. There can, I think, be little doubt that these articles were avidly studied by Faraday. They provided him with a theoretical framework for his work on optical glass and at the same time offered a clue to the problem of the transmission of force without the translation of matter. This was, after all, what the undulatory theory of light was all about. It did, to be sure, rely upon an imponderable fluid—the ether—but given this, Fresnel showed how all optical phenomena could be accounted for by waves, not corpuscles, moving through space. Might not the same model be used to advantage in electricity?

Faraday's concern with waves was not restricted, at this time, to light waves. In the years from 1828 to 1830 he was actively engaged in expounding some of the laws of wave motion. It was in these years that Charles Wheatstone's studies of sound were reported to an interested audience at the Royal Institution by Faraday. Some of the evenings, it must be admitted, were more entertainment than philosophy. On 15 February 1828, for example, some of Wheatstone's principles were illustrated by a virtuoso performer on the Jew's-harp. And, on 23 April 1829, a man who could whistle two tunes at the same time stunned the audience at the R.I. But, there were curious effects that had deep scientific meaning as well. Of these, perhaps the most important for Faraday were Wheatstone's studies of Chladni figures. These are the figures made by sand or powder upon a steel or glass plate when the plate is bowed by a violin bow. In a Discourse at the R.I. in 1829 Faraday showed how these figures could be produced. He also showed how one could produce the figures on one plate by bowing another one near it. He did not remark it at the time, but this was a case of acoustical induction in which the air served to transmit the force of one vibrating plate to another. Could such an analogy with electrical induction long escape Faraday? The answer would appear to be yes, for there is no reflection of this activity in the lecture room in the laboratory diary. None, that is, until the early part of 1831 when Faraday threw himself feverishly into an intensive

‡ *Ibid.*, 56.

experimental investigation of all types of Chladni figures. These researches were to occupy him for six months and end only a few weeks before his induction ring experiment. He first studied the effect of the density of the surrounding air and showed how this medium determined the kind of patterns that were obtained. He then began to experiment with different media, using water, egg-white and oil upon the plates and observing the patterns thus produced. These patterns fascinated Faraday. In them was to be found the clue to the solution of the problem which had occupied him for a decade. We have seen him trying to detect a ' state ' accompanying the passage of a ' current '. But are not these two mutually exclusive? How, it might be asked, can a transmission of force create a stationary arrangement? The crispations of oil and egg white upon a vibrating plate revealed the answer. Here was wave motion (i.e. a progressive transmission of force) as well as a state created by this transmission. Would not, by analogy, the same kind of explanation apply to electrical phenomena?

So far the connection between electricity and acoustical figures has been one supplied by the author of this article and it should be noted that Faraday never once mentions electricity in his research on Chladni figures. But he did tie the two together in a sealed note he requested to be placed in the safe of the Royal Society in March of 1832. Although this note has been published previously,† it has never received the attention it deserves and is, therefore, worth reproducing here *in extenso*.

> " Certain of the results of the investigations which are embodied in the two papers entitled *Experimental researches in Electricity*, lately read to the Royal Society: and the views arising therefrom, in connexion with other views and experiments, lead me to believe that magnetic action is progressive, and requires time; i.e. that when a magnet acts upon a distant magnet or piece of iron, the influencing cause (which I may for the moment call magnetism) proceeds gradually from the magnetic bodies and requires time for its transmission which will probably be found to be very sensible.
>
> " I think also, that I see reason for supposing that electric induction (of tension) is also performed in a similar progressive time.
>
> " I am inclined to compare the diffusion of magnetic forces from a magnetic pole, to the vibrations upon the surface of disturbed water, or those of air in the phenomena of sound; i.e. I am inclined to think the vibratory theory will apply to these phenomena, as it does to sound and most probably to light.
>
> " By analogy I think it may possibly apply to the phenomena of tension also.
>
> " These views I wish to work out experimentally: but as much of my time is engaged in the duties of my office, and as the experiments will therefore be prolonged, and may in their course be subject to the observation of others; I wish, by depositing this paper in the care of the Royal Society, to take possession as it were of a certain date, and a lone right, if they are confirmed by experiments, to claim credit for the views at that date: at which time as far as I know no one is concious of or can claim them but myself."

It is hardly possible to believe that Faraday only adopted these views *after* the discovery of electromagnetic induction and they may, therefore, be seen as fundamental elements leading to the discovery.

† See Alan Jeffreys, *Michael Faraday, A List of His Lectures and Published Writings*, London, 1960, item 195.

The final aspect of the induction experiment was provided just as Faraday completed his acoustical experiments. In June 1831 his friend Gerritt Moll of Utrecht called his attention to the new electromagnet made by Joseph Henry of Albany, New York. By winding a horseshoe magnet with hundreds of feet of insulated wire, Henry had produced magnetic powers hitherto undreamed of. Henry's magnet could easily lift 700 lb, an extraordinary feat. Moreover, as Moll specifically pointed out, the polarity of this magnet could be changed so rapidly by merely switching the electrical connections that a heavy weight could be retained while the poles were reversed. These facts served to reinforce Faraday's idea that powers of matter, not fluids, were involved for if one accepted Ampère's theory such an instantaneous reversal of polarity was ruled out.

In August 1831 Faraday was in possession of the various elements necessary for his famous induction-ring experiment. He was convinced that electricity and magnetism were waves propagated in a material medium. His work on acoustical figures had revealed *how* such waves could be propagated. The news of Joseph Henry's magnet revealed a new and powerful source of magnetism. In spite of Faraday's silence, we can put these elements together and see the rationale behind the experiment.

In 1821 Faraday had asked himself what the proper form of a magnet should be so that the poles would be closest to the surface. He had been thinking, then, of permanent magnets. Now, in 1831, he was thinking of electromagnets. Of the choices he had listed in 1821—" an oblate or oblong spheroid or a sphere or a very thick ring "—only the ring could lend itself properly to being wound by coils of wire. Hence, in Faraday's words, " Have had an iron ring made (soft iron), iron round and $\frac{7}{8}$ inches thick and ring 6 inches in external diameter. Wound many coils of copper wire round one half, the coils being separated by twine and calico—there were 3 lengths of wire each about 24 feet long and they could be connected as one length or used as separate lengths. By trial with a trough each was insulated from the other. Will call this side of the ring *A*. On the other side but separated by an interval was wound wire in two pieces together amounting to about 60 feet in length, the direction being as with the former coils; this side call *B*."†

The ring guaranteed that the magnetic power would be at a maximum where the secondary coil was wound. But what should happen in this set-up? One must, I think, appeal here to Chladni figures and acoustical induction. Magnetism, as Faraday clearly stated in 1832, was progressive and, like waves, developed in a medium. The medium, in this case, was the iron ring. Just as the bowing of one steel plate could produce patterns of vibration on another, neighbouring, plate, so should the development of ' waves ' by a ' current ' in the primary [*A*] produce a pattern or ' state ' in the secondary through the medium of the iron ring. The secondary [*B*] was thus a *detector* of the wave produced by the primary. The evidence now seems overwhelming that this was how Faraday was thinking. This time, the secondary circuit was connected first so that when the primary circuit was ' made ' the looked-for effect became evident; the *wave* did pass and was detected. Or, in Faraday's own words, " Wave apparently very short and sudden ".‡ Electromagnetic induction, for which so many had

† *Faraday's Diary*, 367.

‡ *Ibid.*, 369.

searched for a decade, had become a scientific fact. Faraday's experiment was a success and his immortality was guaranteed.

And yet he was puzzled for things had not worked out *exactly* as he had planned. It is clear from the Diary that he was looking either for a steady effect—i.e. a continuing induction as with acoustical induction, or for a momentary creation of a ' state ' detectable by the galvanometer. The latter seemed to be what was produced by the primary circuit for the deflection of the galvanometer was a momentary, not a steady, one. So far, so good—until Faraday broke the primary circuit. Then he noticed that a ' current ' in the opposite direction was induced in the secondary. This was totally unexpected and posed questions which the analogy with acoustical induction could not answer. After all, when one *ceased* to bow one plate it did not affect another, neighbouring, plate. It was this anomaly that was to lead Faraday deeper and deeper into the quest for understanding of electric and magnetic force. The result was to be field theory.

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